On the other hand, if natural selection cannot thus operate, all we have to say is that there still remain many other causes adequate to explain the occurrence of physiological selection—to wit, those causes which are concerned in the occurrence of variation in general.

The essay by Prof. Weismann on the influence of isolation, to which Mr. Meldola refers, is so replete with facts and arguments unconsciously bearing on my theory, that in writing my preliminary paper it appeared advisable to reserve so rich a mine for subsequent working out in detail. In my paper, therefore, I have merely alluded to Prof. Weismann as one among the comparatively few evolutionists who have hitherto sufficiently considered the influence of independent variation (or the prevention of intercrossing) in the evolution of species.

It only remains to consider Mr. Meldola's extremely able criticism of my view that natural selection ought not in strictness to be regarded as a theory of the origin of species, but rather as a theory of the development of adaptive modifications. My argument is that natural selection can only be a theory of the origin of species in so far as species differ from one another in points of utilitarian significance; and that even then it is only a theory of the origin of species, as it were, incidentally: the *raison d'être* of natural selection is in all cases that of evolving adaptations (whether these be characteristic of species only, or likewise of higher taxonomic divisions); and if in some cases the result of performing this function is that of raising a variety into a species, such a result is merely collateral, or, in a sense, accidental. No doubt if species always and only differed from one another in points of utilitarian character, the collateral nature of the result might be disregarded, and the theory would become a theory of the origin of species in virtue of its being a theory of the development of adaptations. But, as a matter of fact, species are very far from being always and only distinguished from one another in points of utilitarian character, and in so far as they are not thus distinguished natural selection is obviously in no sense a theory of the origin of species. Again, and more particularly, the one feature which more than any other serves to distinguish species from species is that of mutual sterility, and it would be a bold flight of speculation to affirm that this has been in all cases the result of natural selection, when even Mr. Darwin was reluctantly compelled to conclude that such could not be the result of natural selection in any case. On the other hand, my theory of physiological selection explains this very general feature of specific distinction quite independently of natural selection; and then goes on to show that, when once this primary distinction has arisen, many others of a secondary kind will ensue, both with and without the assistance of natural selection.

Now, the objection which Mr. Meldola adduces against this argument is that I have not proved physiological selection to be independent of natural selection. In other words, he does not dispute the probable truth of my theory; but he says that, granting its truth, it is still only "one particular phase of natural selection." But surely the burden of proof here lies on the side of my critic. If he can show any sufficient reason for going much further than I have ventured to go in out-Darwining Darwin—or for holding that natural selection may not merely help in inducing sterility in some cases, but has been the sole cause of it in all cases—then I should welcome his proof as showing that the principles of physiological selection ultimately and in all cases rest on those of natural selection. But, clearly, it is for him to prove his positive: not for me to prove what I regard as an almost preposterous negative.

So much for the main criticism. But he adds this rider, namely, that, as the struggle for existence is always most severe between the most closely related forms, unless the new or sexually protected form arising under physiological selection possesses some distinct advantage over the old or parent form, it will be exterminated by the latter quite as effectually as it would be by intercrossing in the absence of physiological selection. To this I may answer in the words of my full paper:—"So long as there is no actual *detriment* arising to the variety on account of its being sexually separated from the parent, any ideas derived from the theory of natural selection are plainly without bearing upon the subject" (p. 406). In other words, so long as in all other respects of organisation the sexually separated variation is not less "fit" than its parent stock, so long there is no reason to anticipate any disadvantage in the struggle for existence. And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms—mere numerical inferiority being without significance where intercrossing is prevented. It was in order to convey this meaning that I proposed as an alternative name of my theory, "Segregation of the Fit"; seeing that before any physiological segregation can take place there must be organisms to be segregated, and that unless these organisms had already proved themselves fitted to survive in the struggle for existence, in existence they could not be. But I do not call physiological selection "Segregation of the *Fittest*," because, unlike natural selection, it is in no way concerned with the principle of conflict. So long as the organisms which have been separated by physiological selection are sufficiently fit to have previously passed muster at the hands of natural selection, there is no reason why the daughter type should be fitter than the parent.

But, so far as I can see, the only material point of difference between Mr. Meldola and myself consists in his regarding physiological selection as "subordinate" to natural selection, while I consider the two as quite independent principles, although, as explained in my paper, I believe that they frequently and in several ways play into each other's hands.

George J. Romanes

Geanies, Ross-shire, N.B., August 30

Earth-Currents and Aurora

There appears to have been a very remarkable and widespread earth-current storm on March 30 last, full particulars of which it would be extremely useful to have on record. My attention has been drawn to this storm through witnessing, on the evening of that day, one of the most vivid and interesting displays of the aurora that I have ever seen. Mr. G. H. Kinahan, writing in Nature for April 8 (p. 537), describes the same aurora as observed by him in Donegal between 8 and 9 p.m., and notes its peculiar bright silvery type. It must, however, have been a far less imposing display at Donegal, where the weather was less favourable, than at Kingstown, where I saw it between 9 and 10 p.m., the most brilliant display occurring between 9.30 and 9.45 p.m. From the northerly horizon there rolled up to the zenith in quick succession strea[illegible] and masses of white light, until the whole face of the sky to the north and west was illuminated with swiftly mounting flames of silvery whiteness and wonderful beauty. A correspondent in Nature for April 15 (p. 559) describes the same aurora witnessed by him between 8 and 11 p.m. on March 30, at Königsberg, in Prussia.

According to the *Electrician* (April 2, p. 404), on the morning of March 30 a violent earth-current storm occurred in London, stopping all telegraphic work for some time. During the same day strong earth-currents are reported on the Mediterranean cables, and in the afternoon of that day (March 30) this terrestrial electric storm had reached India, lasting from 2 to 5 p.m., and stopping work on the Bombay and Madras line. At the same time powerful earth-currents are reported on the Madras and Penang cable, causing work over it to be stopped from 3 till 7 p.m. on March 30. Similar disturbances are reported on the Java cable, beginning the same afternoon, and becoming fainter on all lines at 10 p.m. Recently an account has been published of a great earth-current storm on the China and Japan cable on March 30, and a diagram of the perturbations produced on that line is given in the *Electrician* for August 6. The storm began between 4 and 5 p.m., Shanghai time, on March 30, and lasted till 11 p.m. that day, the maximum strength of the earth-current on the cable occurring between 6 and 7 p.m. being then equal to 3·6 milliamperes. Smaller renewals of the storm took place both in Europe and Asia on the morning of March 31.

Perhaps some of the readers of Nature can give further details of this extensive storm, and it would also be useful to have on record the magnetic perturbations noticed on this occasion in different localities, and the time of their occurrence.

W. F. Barrett

Royal College of Science, Dublin

Chlamydomyxa in the Engadine

Your readers will be interested to hear that I have found here in Pontresina the very interesting Protozoon described twelve years ago, in the *Quarterly Journal of Microscopical Science*, by

Mr. Archer, of Dublin, under the name Chlamydomyxa. Mr. Archer obtained his specimens crawling upon, and into, the tissues of the bog-moss (Sphagnum) in moor-pools in Westmeath (so far as I recollect). He often found it in a spherical condition, encysted in a cellulose envelope, and more rarely expanding into a most curious network of fine protoplasmic threads, upon which were observed very numerous oblong corpuscles, which slowly travelled along the threads, thus recalling the structure of Labyrinthula described by Cienkowski.

No one has found Archer's Chlamydomyxa, or been in a position to confirm independently his description, up to the present date. He has kindly sent to me *encysted* specimens of the Chlamydomyxa, but these would not leave their cysts and exhibit the characteristic network and corpuscles; and others whom he has wished to oblige by a sight of this interesting organism have also, I believe, failed to obtain the characteristic phase.

I have from time to time searched for Chlamydomyxa when I have been in a moorland region and had my microscope with me, but have hitherto failed to find it. It was not, therefore, with any great amount of confidence that I gathered some brownish tufts of Sphagnum from a small ditch (with slowly-running water) in a clearing in the pine-wood behind the tennis-court at Pontresina, and brought them to the hotel to search them for Chlamydomyxa. But I found a number of yellowish spheres about 1/150 inch, and less in diameter, which excited my suspicions. After a brief delay these began to throw out protoplasmic filaments, and soon around each was a wonderful series of branching stems of protoplasmic threads, reaching far away from the central yellow granular body and in the most varied directions. Along the threads minute oval corpuscles slowly streamed. There is no doubt that this organism belongs to Archer's genus Chlamydomyxa, and probably enough is specifically distinct from that which he found in Ireland. It is extremely abundant in the Swiss locality.

From what I have seen of Chlamydomyxa, I am now inclined to admit that it is *less* closely related to Cienkowski's Labyrinthula than I had previously supposed. The moving corpuscles of Cienkowski's organism are very much larger bodies than are the ovoid corpuscles of Chlamydomyxa.

Pontresina, August 23 E. Ray Lankester

THE BRITISH ASSOCIATION

THE fifty-sixth annual meeting of the British Association was opened in Birmingham last night, when Lord Rayleigh resigned the Presidential chair to Sir William Dawson, Principal of McGill College, Montreal. The attendance at the fourth Birmingham meeting promises to be much above the average, and so far as the Birmingham people are concerned everything has been done to secure success. All the new public buildings, including Mason College and the fine new Art Gallery, have been placed at the disposal of the Sections. The reception-rooms, reading-rooms, writing-rooms, and other general rooms, not omitting the smoking-room, are all that could be desired. All the leading clubs of the town have been thrown open to members; facilities have been given for visiting the various manufactories in the town and neighbourhood; the industrial exhibition in Bingley Hall has been admirably arranged; while there is a formidable programme of social entertainments and excursions. Among the foreign visitors expected, one of the most distinguished is Prof. Haeckel. There is a very large representation, moreover, of colonial science.

The Local Committee have prepared a very excellent Hand-Book of Birmingham for the use of the visitors, the various sections of which are written by specialists. After a General Introduction by Mr. G. J. Johnson, Part 1 deals with Old Birmingham, by Mr. S. Timmins. The various aspects of Modern Birmingham are treated of by different writers. The section on Art is by four specialists, and the Manufacturing Industries are described by Mr. C. J. Woodward. Part 3 is devoted to Geology and Physiography, and is edited by Prof. C. Lapworth, who has been assisted in the various sections by several local geologists. Part 4, dealing with Zoology, is edited by Mr. W. R. Hughes, F.L.S., who also has had the assistance of various local specialists; so with the Botany, edited by Mr. W. Mathews. The appendix comprises a variety of curious and useful information, while a pocket contains a useful sketch-map of the Geology of the Birmingham district, by Prof. Lapworth. It will thus be seen that the Hand-Book, while well adapted for its special purpose, is likely to be of permanent value.

Inaugural Address by Sir J. William Dawson, C.M.G., M.A., LL.D., F.R.S., F.G.S., Principal and Vice-Chancellor of McGill University, Montreal, Canada, President

Twenty-one years have passed away since the last meeting of the British Association in this great central city of England. At the third Birmingham meeting—that of 1865—I had the pleasure of being present, and had the honour of being one of the Vice-Presidents of Section C. At that meeting my friend John Phillips, one of the founders of the Association, occupied the Presidential chair, and I cannot better introduce what I have to say this evening than by the eloquent words in which he then addressed you:—"Assembled for the third time in this busy centre of industrious England, amid the roar of engines and clang of hammers, where the strongest powers of nature are trained to work in the fairy chains of art, how softly and fittingly falls upon the ear the accent of Science, the friend of that art, and the guide of that industry! Here where Priestley analysed the air, and Watt obtained the mastery over steam, it well becomes the students of nature to gather round the standard which they carried so far into the fields of knowledge. And when on other occasions we meet in quiet colleges and academic halls, how gladly welcome is the union of fresh discoveries and new inventions with the solid and venerable truths which are there treasured and taught. Long may such union last; the fair alliance of cultivated thought and practical skill; for by it labour is dignified and science fertilised, and the condition of human society exalted." These were the words of a man who, while earnest in the pursuit of science, was full of broad and kindly sympathy for his fellow-men, and of hopeful confidence in the future. We have but to turn to the twenty Reports of this Association, issued since 1865, to see the realisation of that union of science and art to which he so confidently looked forward, and to appreciate the stupendous results which it has achieved. In one department alone—that to which my predecessor in this chair so eloquently adverted in Aberdeen, the department of education in science—how much has been accomplished since 1865. Phillips himself lived to see a great revolution in this respect at Oxford. But no one in 1865 could have anticipated that immense development of local schools of science of which your own Mason College and your admirable technical, industrial, and art schools are eminent examples. Based on the general education given by the new system of Board-schools, with which the name of the late W. E. Forster will ever be honourably connected, and extending its influence upward to special training and to the highest University examinations, this new scientific culture is opening paths of honourable ambition to the men and women of England scarcely dreamed of in 1865. I sympathise with the earnest appeal of Sir Lyon Playfair, in his Aberdeen address, in favour of scientific education; but, visiting England at rare intervals, I am naturally more impressed with the progress that has been made than with the vexatious delays which have occurred, and am perhaps better able to appreciate the vast strides that have been taken in the direction of that complete and all-pervading culture in science which he has so ably advocated.

No one could have anticipated twenty years ago that a Birmingham manufacturer, in whose youthful days there were no schools of science for the people, was about to endow a College, not only worthy of this great city, but one of its brightest ornaments.[1] Nor could any one have foreseen the great development of local scientific Societies, like your Midland Institute and Philosophical Society, which are now flourishing in every large town and in many of those of less magnitude. The period of twenty-one years that has elapsed since the last Birmingham meeting has also been an era of public museums and laboratories for the teaching of science, from the magnificent national

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[1] It was in 1865 that Sir Josiah Mason was, quietly and without any public note, beginning to lay the foundation of his orphanage at Erdington.